

IN THE UNITED STATES DISTRICT COURT FOR THE DISTRICT OF MARYLAND

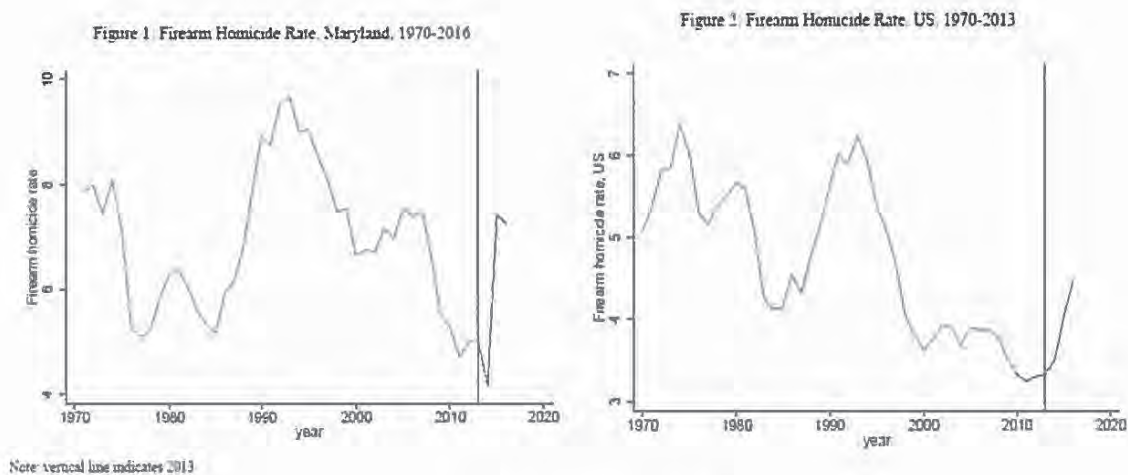
MARYLAND SHALL ISSUE, INC., et)	
al.;)	
)	
Plaintiffs,)	
)	
v.)	Case No.: 16-cv-3311-ELH
)	
LAWRENCE HOGAN, et al.;)	
)	
Defendants.)	
)	

DECLARATION OF CARLISLE MOODY

I, Carl Moody, under penalty of perjury, declare and state as follows:

1. I am more than 18 years of age and am competent to testify, upon personal knowledge, to the matters stated below.
2. I am qualified to offer my expert opinions. I am a professor of economics at the College of William and Mary, where I have taught econometrics, mathematical economics, and time series analysis for 48 years. I was chair of the Economics Department from 1997–2003. I earned my Ph.D. in economics in 1970, and I have published over 40 peer-reviewed articles. I have researched guns, crime, and gun policy for almost 20 years and published 12 articles related directly to these topics.
3. A copy of my expert report in this matter is attached hereto as Attachment A, the contents of which are, to the best of my knowledge and belief, true and accurate. I hereby adopt and incorporate that report as if set forth fully herein and in this declaration I attempt to give a higher level view of my analyses and conclusions.

4. For this case, I wanted to know whether the Firearm Safety Act (the “FSA”), specifically the Handgun License requirement, which contains the handgun qualifying license (“HQL”) requirement, has impacted Maryland’s firearm homicide rate. As can be shown in the two illustrations immediately below, the firearm homicide rate is increasing throughout the United States. Therefore, I examined whether the FSA caused Maryland’s firearm homicide rate to increase less than it would have without the FSA. I used four different methods to study the FSA’s impact. Each method revealed that the FSA has had no beneficial impact on Maryland’s firearm homicide rate.



Methodology #1: t-test

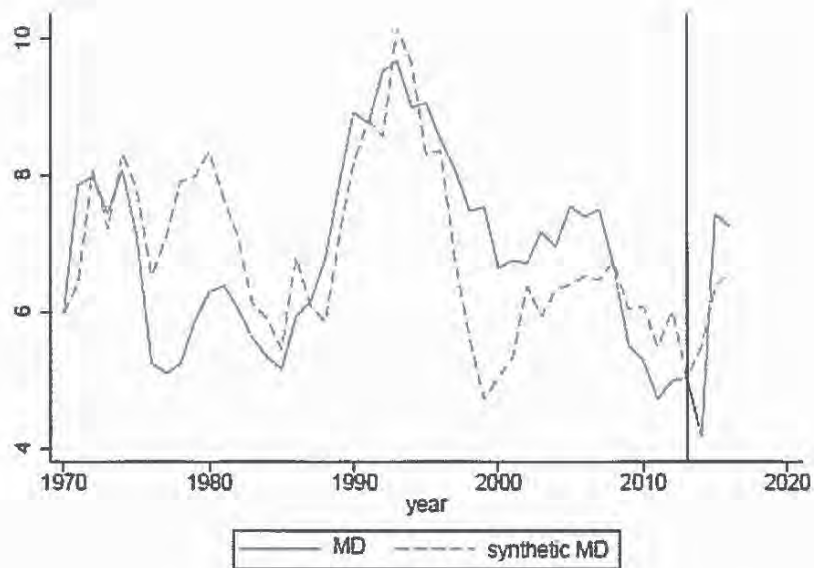
5. First, I compared the firearm homicide rates between Maryland, which requires its residents to obtain a handgun qualify license before they can purchase a handgun (the HQL), and states that do not have a permit or license requirement. The non-permit states’ firearm homicide rate increased by 10.01 percent since 2013. By contrast, Maryland’s firearm homicide rate surprisingly has increased by 20.68 percent – more than twice as much – since 2013. I tested the significance of this observation using a very simple statistical methodology known as a t-test. At a basic level, t-tests essentially compare the averages of two sets of data (here Maryland’s firearm

homicide rate after 2013 and all other non-permit state's firearm homicide rate) and tells the researcher if those differences could have happened by random chance or instead are significantly different from one another. The results of this test indicated a high enough P-value (the measure of the probability that the results in your sample occurred by chance) that I cannot say that these differences did not occur by chance. Thus, the data here indicate that the firearm homicide rate in Maryland has increased more in Maryland since it started requiring the HQL than in other states that do not have a similar licensing law, but the data do not allow me to conclude that these differences did not simply happen by chance or that the HQL is causing an increase in the firearm homicide rate.

6. Without a definitive answer after the first methodology I turned to three other more complex methodologies: the synthetic control method; the difference in differences method; and the fixed-effects panel data method.

Methodology #2: The Synthetic Control Method

7. The synthetic control methodology is a technique to estimate what would have happened if something did or did not happen. Here, I created a "Synthetic Maryland" that represents Maryland if it had not started requiring Handgun Licenses in 2013. I then compared Synthetic Maryland's firearm homicide rate to real Maryland's firearm homicide rate. Synthetic Maryland had a fire arm homicide rate since 2013 of 6.14 and real Maryland had a firearm homicide rate of 6.28 per 100,000. Again, like the simple t-test above, this suggests that at a minimum the Handgun License did not reduce the firearm homicide rate.



8. One common issue with the synthetic control method determining whether Synthetic Maryland (Maryland without the FSA) has a lower firearm homicide rate than Maryland (with the FSA) because of random chance or whether the FSA is actually *increasing* firearm homicides in Maryland.

9. One econometric solution to this problem is to use a falsification test. What I did here was to essentially reassign the FSA to other states in the original control group (as if these states adopted it in 2013 rather than Maryland). I looked at what the synthetic versions of each of those states were predicted to be (without adopting the FSA) and compared them to what they actually were. I then took the differences between the synthetic and real (*i.e.*, post 2013 data) and compared it with the gap between Synthetic Maryland and real Maryland and looked for statistical differences between the Maryland gap and all the other gaps. If the Synthetic Maryland gap was much larger or smaller than the other gaps that would be a good indication that the FSA had a positive or negative effect on firearm homicide in Maryland.

10. The results of the falsification test showed the average gap between Maryland and Synthetic Maryland from 2013-16 compared with the other non-permit states and their synthetic state gaps were essentially the same. This indicates that the FSA has had no significant effect in either direction on the firearm homicide rate.

Methodology #3: The Difference in Differences Method

11. The difference in differences method is a research methodology that allows researchers to look at the differences between groups over time to calculate the effect of a particular intervention and minimize concerns over omitted variable bias. This methodology is probably most easy to explain with a simplified example. Suppose you are a farmer who in 2016 grew 10,000 ears of corn. In 2017 you used a new type of fertilizer and you grew 15,000 ears of corn. One cannot safely conclude that the new fertilizer increased the production of the farm by 5,000 ears of corn because there are other factors that may have played a part in the additional ears of corn. One way of figuring out the effect of the fertilizer would be looking at your neighbor's farm. If his farm is similar, he didn't use the new fertilizer, and he also produced more ears of corn of his farm, then perhaps favorable weather in 2017 or some other unknown variable could explain some of the increase in productivity. But if his percentage increase in productivity was less than yours, the delta between your increase and your neighbors might be attributed to the fertilizer.

12. This is essentially what I did here except with many more observations and more rigorous statistical analysis. I looked at Maryland's firearm homicide rate before and after Maryland passed the FSA. I then compared those rates with the firearm homicide rates of other similar, geographically close states (Delaware, Virginia, Pennsylvania, and also ran the comparison with Synthetic Maryland from the experiment above) that did not pass permit-to-

purchase (“PTP”) laws but would likely be subject to many of the same other variables that could also be affecting that firearm homicide rate over the same period of time.

13. The result was again no significant difference between any of the states and Maryland. This methodology suggests that Maryland’s firearm homicide rate was not affected in either direction by Maryland’s passage of the FSA.

Methodology #4: Fixed-Effects Panel Data Method

14. Finally, I used the fixed-effects panel data method to specifically focus on all states that have passed PTP laws between 1970 and 2016 (including Maryland) to see if those laws have had any effect on the firearm homicide rates within those states. I did this by analyzing data on the homicide rates in each state that at some point passed a PTP law for each year in the relevant period as well as other data on other variables in each state in that time span that research suggests may have impacted each of those state’s firearm homicide rates (such as three strikes laws, poverty rate, castle doctrine, and drug and alcohol use). I then ran a regression that essentially collects all that data and predicts the effect of each of those variables (including PTP laws) on the firearm homicide rate. I also did this same experiment adding a lagged dependent variable to correct for any unobservable effects that are not permanent. Based on all the prior experiments it should come as no surprise that the coefficient for PTP both with and without the lagged dependent variable was not statistically significantly different from zero. In sum, PTP laws across the six states that enacted them between 1970-2016 had no significant effect on firearm homicide rates.

Table 2 Fixed effect panel data regression: firearm homicide rate

Variable	Coeff	t-ratio	Coeff	t-ratio
Permit to purchase	-0.718	1.85	0.092	0.72
Prison population per capita, lagged	0.012	0.03	-0.023	0.21
Police officers per capita, lagged	-0.583	0.92	-0.216	1.30
Cutions	-0.022	1.15	-0.005	0.98
Crack epidemic measure	0.316	2.05*	0.132	1.49
Beer consumption per capita	0.048	2.05*	0.014	2.14*
Truth in sentencing	-0.180	0.70	-0.108	0.78
Three strikes	0.562	1.12	0.070	0.46
Right to carry	0.156	0.69	0.001	0.01
Castle doctrine	0.034	0.12	0.083	0.90
Stand your ground	0.049	0.12	0.091	0.66
State large capacity magazine ban	-0.088	0.13	0.217	0.98
Use a gun go to jail	0.117	0.48	0.018	0.16
Background check, handgun	-0.841	1.27	-0.236	1.62
Safe storage law	-0.147	0.29	0.111	0.72
Juvenile gun ban	0.386	1.03	0.273	1.63
One gun per month	-0.349	0.57	-0.050	0.33
Sat. Night special ban	0.681	0.53	-0.019	0.06
State assault weapon ban	-0.556	0.74	0.024	0.16
Background check, private sale	0.377	1.40	0.110	1.16
Real income pc	-0.170	1.24	-0.124	2.50*
Real welfare payments per capita	1.181	0.49	-0.805	0.67
Poverty rate	0.079	2.79**	0.014	0.94
Percent black	0.700	3.24**	0.169	2.91**
Unemployment rate	-0.020	0.26	-0.031	1.15
Employment per capita	0.170	1.03	0.029	0.60
Military employment per capita	0.537	0.46	0.229	0.77
Construction employment per capita	0.359	1.40	0.255	1.86
Firearm homicide rate, lagged			0.786	17.00**
N	2,270		2,264	

Notes: * p < 0.05; ** p < 0.01, also included but suppressed to conserve space: eleven demographic age groups, year dummies, and individual state trends. Complete results in Appendix B.

15. However, due to there only being 6 policy changes in that time period to focus on, standard errors could be underestimated and t-ratios could correspondingly be overestimated, which can make determinations of significance problematic. I chose five random states to adopt PTP laws in random years and one random state to repeal its law in a random year and did this 1,000 times. Because we know that those laws are fictitious and randomly assigned, it allows us to safely assume that those coefficients and t-ratios are centered around zero. This allows me to

calculate the p-values corresponding to the t-statistics in Table 2 above, and again led to the conclusion that PTP laws have had no statistically significant effect on firearm homicide rates.

16. Another possible issue to study to further analyze these results is the possibility of a problem with serial correlation. Serial correlation is an issue when one observes the same variable over a period of time and a prior observation effects the present/future observations, or to put it another way, the issue is that the observations are not random. I conducted several tests to determine if there were any issues of serial correlation with firearm homicide rates in the data above. The results were mixed. I therefore, as a robustness check, re-ran the above experiment but this time taking first differences (the changes in the values of the variable from one year to the next) as this is the standard cure if a time series data set has issues with serial correlation. As can be seen below the results are the same, and the PTP variable was still not significantly different from zero.


Table 3: Fixed effects model: firearm homicide rate, first differences

Variable	Coeff	t-ratio	Coeff	t-ratio
Permit to purchase	-0.005	0.01	-0.035	0.11
Prison population per capita, lagged	0.215	1.26	0.221	1.26
Police officers per capita, lagged	0.050	0.49	0.053	0.51
Executions	0.035	3.19**	0.036	3.25**
Crack epidemic measure	-0.006	2.72**	-0.006	2.50*
Beer consumption per capita	-0.000	0.14	-0.000	0.23
Truth in sentencing	-0.009	3.03**	-0.009	3.28**
Three strikes	0.006	0.05	0.004	0.04
Right to carry	0.147	0.85	0.156	0.92
Castle doctrine	0.075	0.41	0.086	0.47
Stand your ground	-0.008	0.04	-0.021	0.12
State large capacity magazine ban	-0.531	3.19**	-0.532	3.27**
Use a gun go to jail	0.003	0.02	-0.008	0.04
Background check, handgun	-0.131	0.39	-0.148	0.43
Safe storage law	-0.046	0.16	-0.058	0.19
Juvenile gun ban	0.280	1.54	0.288	1.60
One gun per month	-0.098	0.31	-0.130	0.40
Sat. Night special ban	-0.116	0.47	-0.124	0.49
State assault weapon ban	0.128	0.45	0.131	0.45
Background check, private sale	0.299	1.15	0.312	1.13
Real income pc	-0.202	2.38*	-0.206	2.37*
Real welfare payments per capita	-0.402	0.25	-0.393	0.25
Poverty rate	0.001	0.04	0.002	0.13
Percent black	0.462	2.16*	0.492	2.35*
Unemployment rate	0.008	0.30	0.014	0.51
Employment per capita	0.024	0.37	0.027	0.43
Military employment per capita	0.001	0.00	0.037	0.08
Construction employment per capita	0.749	4.01**	0.749	4.03**
Firearm homicide rate, lagged			-0.059	0.82
N	2,212		2,211	

Notes: * $p < 0.05$, ** $p < 0.01$; also included but suppressed to conserve space: eleven demographic age groups, year dummies, and individual state trends. Complete results are shown in Appendix B.

17. The above research all leads me to the singular conclusion that the passage of the FSA has not reduced the firearm homicide rate in Maryland.

I declare and affirm under penalty of perjury that the foregoing is true and correct to the best of my knowledge, information, and belief.


Carlisle Moody

10/3/2018
Date

I. INTRODUCTION

I am Dr. Carlisle E. Moody, Professor of Economics at the College of William & Mary. I have been asked an opinion regarding this case. This report sets forth my qualifications, opinions, and scholarly foundation for those opinions.

II. BACKGROUND & QUALIFICATIONS

I am a Professor of Economics at the College of William and Mary in Virginia. I graduated from Colby College in 1965 with a major in Economics. I received my graduate training from the University of Connecticut, earning a Master of Economics degree in 1966 and a Ph.D. in Economics in 1970, with fields in mathematical economics and econometrics.

I began my academic career in 1968 as Lecturer in Econometrics at the University of Leeds, Leeds, England. In 1970 I joined the Economics Department at William and Mary as an Assistant Professor, I was promoted to Associate Professor in 1975 and to full Professor in 1989. I was Chair of the Economics Department from 1997-2003. I am still teaching full time at William and Mary. I teach undergraduate and graduate courses in Econometrics, Mathematical Economics, and Time Series Analysis.

I have published over 40 refereed journal articles and several articles in law journals and elsewhere. Nearly all these articles analyze government policies of various sorts. I have been doing research in guns, crime, and gun policy since 2000. I have published 12 articles directly related to guns and gun policy.

I have also consulted for a variety of private and public entities, including the United States Department of Energy, U.S. General Accountability Office, Washington Consulting Group, Decision Analysis Corporation of Virginia, SAIC Corporation, and the Independence Institute.

A full list of my qualifications, as well as a list of my publications, is attached hereto as **Exhibit 1**.

In the past four years, I have written expert reports, been deposed, or testified at trial in the following matters:

- *Cooke v. Hickenlooper*, U.S. Dist. Ct., Dist. of Colo., Oct. 25, 2013 (submitted expert report, not deposed, did not testify);
- *Rocky Mountain Gun Owners v. Hickenlooper*, Dist. Ct., City and County of Denver, Case No. 2013-CV-33897, May 1, 2017 (testified).
- *William Wiese, et al v. Becerra*, U.S. Dist. Ct., E. Dist. of Cal., Case No. 2:17-cv-00903-WBS-KJN, April 28, 2017 (submitted expert report, not deposed, did not testify).

- *Duncan, et al. v. Becerra, et al.* United States District Court (S.D. Cal.) Case No: 3:17-cv-01017-BEN-JLB, March 26, 2018 (submitted expert report, deposed, did not testify).

III. COMPENSATION

I am being compensated for my time in this case at an hourly rate of \$300 per hour. My compensation is not contingent on the results of my analysis or the substance of my testimony.

IV. ASSIGNMENT

Plaintiffs' counsel has asked me to provide an opinion in this case

Expert Witness Report of Dr. Carlisle E. Moody

Introduction

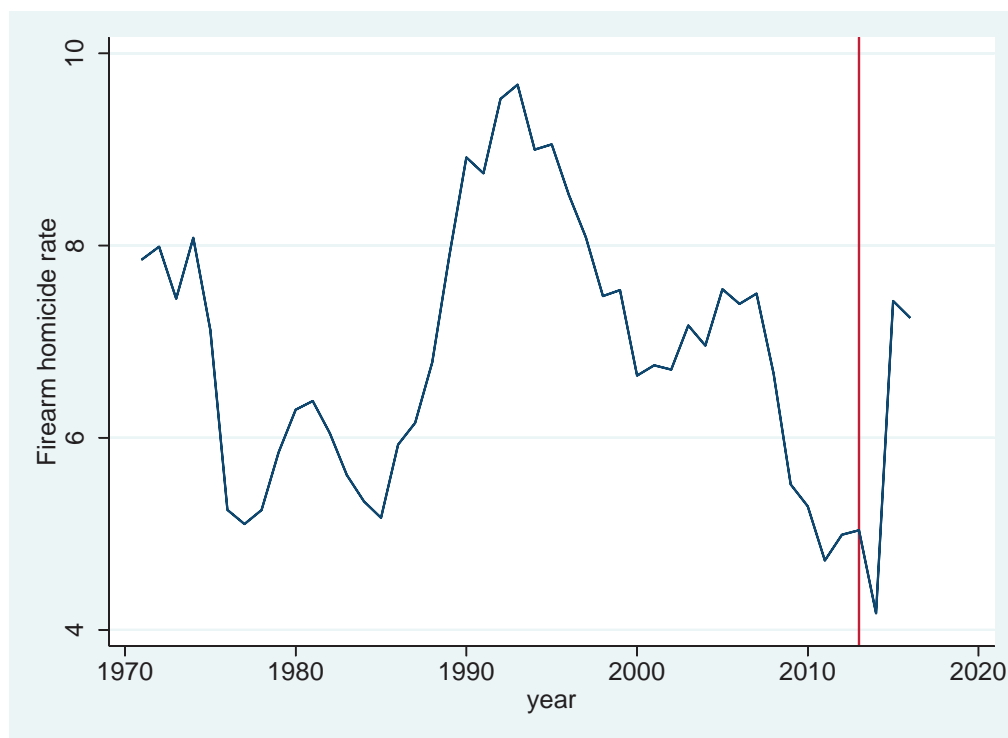
I was pleased to learn, from reading the article written by Professor Webster, et al (Crifasi, Buggs, Choksy, and Webster, 2018) that Maryland's Firearm Safety Act (FSA) of 2013 has significantly lowered the number of crime handguns in Baltimore.

The FSA was associated with an 82 percent reduction in the number of handguns originally sold in Maryland that were recovered within one year of retail sale and the purchaser was not the same as the possessor ($IRR=.18$, $p=.005$); this is a key indicator that a gun was purchased with the intent of diverting it for criminal use. (Crifasi, et al 2018, pp. 133-135)

This amazing success led me to wonder how the criminals are getting by with less than 20 percent of their guns. That is, how much has gun crime been reduced as a result of this astonishing reduction in crime guns?

Firearm homicide is the most accurately measured gun crime. I collected data from the CDC on firearm homicide for Maryland from 1970 to 2016.¹ The Maryland firearm homicide rate per 100,000 population is shown in Figure 1.

Figure 1: Firearm Homicide Rate, Maryland, 1970-2016



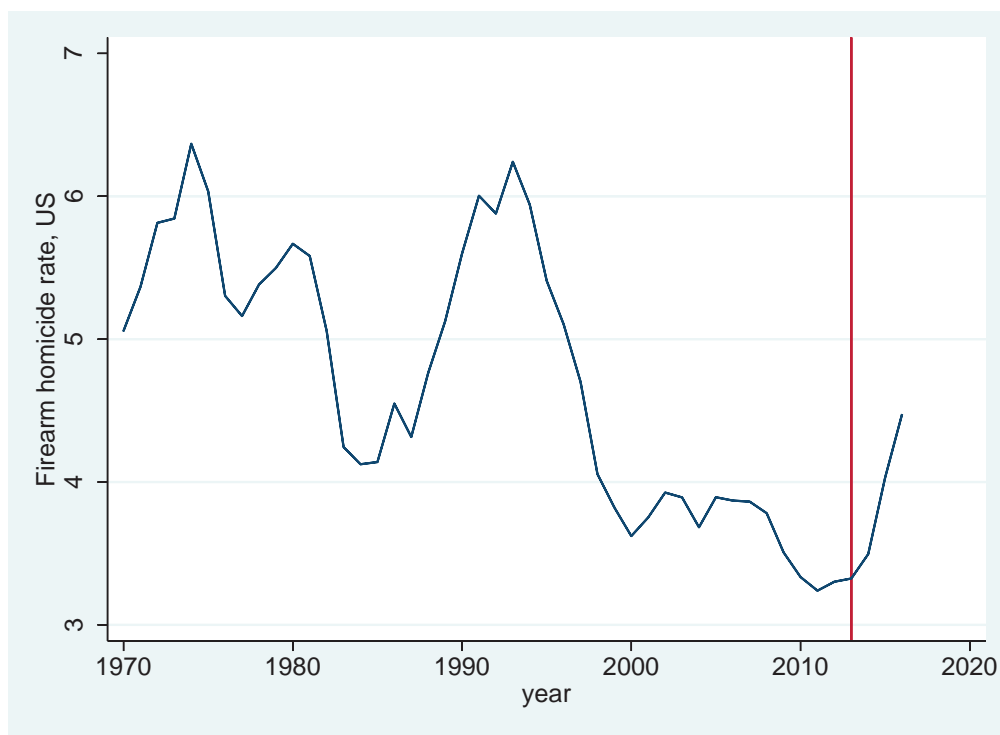
Note: vertical line indicates 2013.

¹ Although it includes justifiable homicides, which are not crimes.

I was shocked to see that the firearm homicide rate in Maryland has increased markedly since the FSA was implemented in 2013. How could this be, given an 82% reduction in crime guns? Perhaps crime is up overall and the trend in Maryland is merely reflective of that general trend.

I collected data on firearm homicide rates for all 51 states, including DC, and averaged across years for the sample period. The results are shown in Figure 2.

Figure 2: Firearm Homicide Rate, US, 1970-2013



It appears that firearm homicide rates are, in fact, up overall, perhaps because of the “Ferguson Effect.”²

Now the question is: Did the FSA reduce gun homicides in Maryland such that the increase in gun homicide rates in Maryland is less than what would have happened in the absence of the FSA? This is a more difficult question to answer because it requires us to construct a counterfactual.

One counterfactual that might be appealing is to determine whether the increase in gun murders in Maryland is greater or less than in other, non-permit, states. One would expect that, with an 82% reduction in crime guns, Maryland would have experienced an increase in the firearm homicide rate significantly less than the growth experienced by non-permit states. Thus, if

² https://en.wikipedia.org/wiki/Ferguson_effect

firearm homicide rates in Maryland do not increase significantly less than those in non-permit states, we would have to conclude that the law has failed in reducing gun murders.

For this purpose I calculated the percent change of the firearm homicide rate after 2013 for Maryland and the non-permit states. The growth rate for Maryland is 20.68 percent. This is more than twice as high as the growth rate for the non-permit states (10.01 percent). However, these growth rates are not significantly different from each other using a standard t-test ($p=.40$). (Appendix A has a primer on statistical methodology for readers unfamiliar with the concepts and terminology. Appendix B has log files containing the output of the various procedures. Stata 15 is the software.)

So, despite the apparent 82% decline in crime guns, Maryland has not experienced a slower rate of growth in firearm homicide rates compared to states that do not have permit to purchase laws.

Synthetic control method

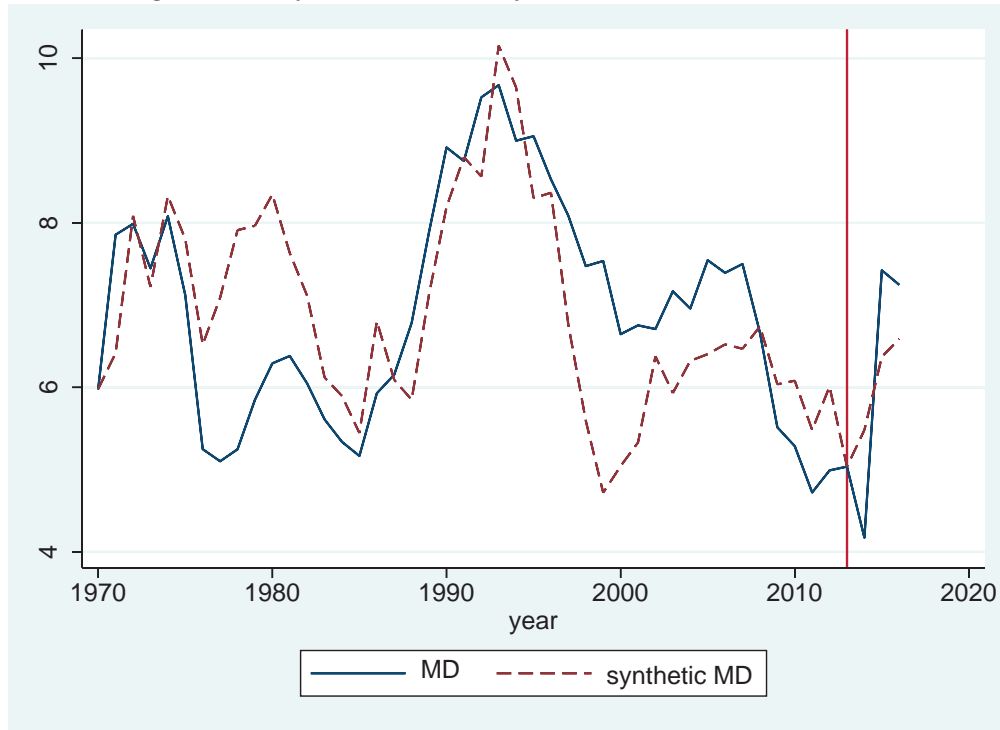
An alternative approach is the synthetic control methodology of Abadie, Diamond, and Hainmueller (2010). This technique uses a weighted average of a subgroup of the control states (states that did not adopt permit laws) to estimate the counterfactual. In the current application, Maryland's experience with firearm homicide since 2013 is compared to a synthetic control state computed by matching a combination of the control states with Maryland using a sophisticated matching algorithm. The control states are those that have never implemented a permit to purchase law. However, for six control states the CDC has suppressed the number of firearm homicides because there were so few: ME, NH, ND, SD, VT, and WY. Six states changed their permit laws in the sample period: CT, IA, MN, MD, MO, and NE. In addition, HI, IL, MA, MI, NJ, NY, NC, and RI have had permit laws since before 1970.³ DC is not included because it is a city, not a state. Thus, I have the remaining 30 states to use as the donor pool of control states for the synthetic control method. The matching algorithm produced a synthetic Maryland equal to $.321*CA+.337*DE+.341*LA$.

Synthetic Maryland predicted Maryland's actual firearm homicide rate with a 14.6 absolute percent error over the period 1970-2013. Because the synthetic firearm homicide rate is above the actual homicide rate in 2013, I adjusted the synthetic rate to be equal to the actual rate in 2013. Figure 3 shows Maryland's actual firearm homicide rate and the firearm homicide rate for the synthetic control state.⁴

³ My primary source for the dates of these laws is Vernick and Hepburn (2003). However, Thomas B. Marvell, PhD, JD has found two small errors in that article concerning the dates for Missouri, and the fact that Rhode Island has had a permit law since before 1970. These corrections are reflected in the dates that I use in this report.

⁴ Appendix B has the relevant log file with complete results.

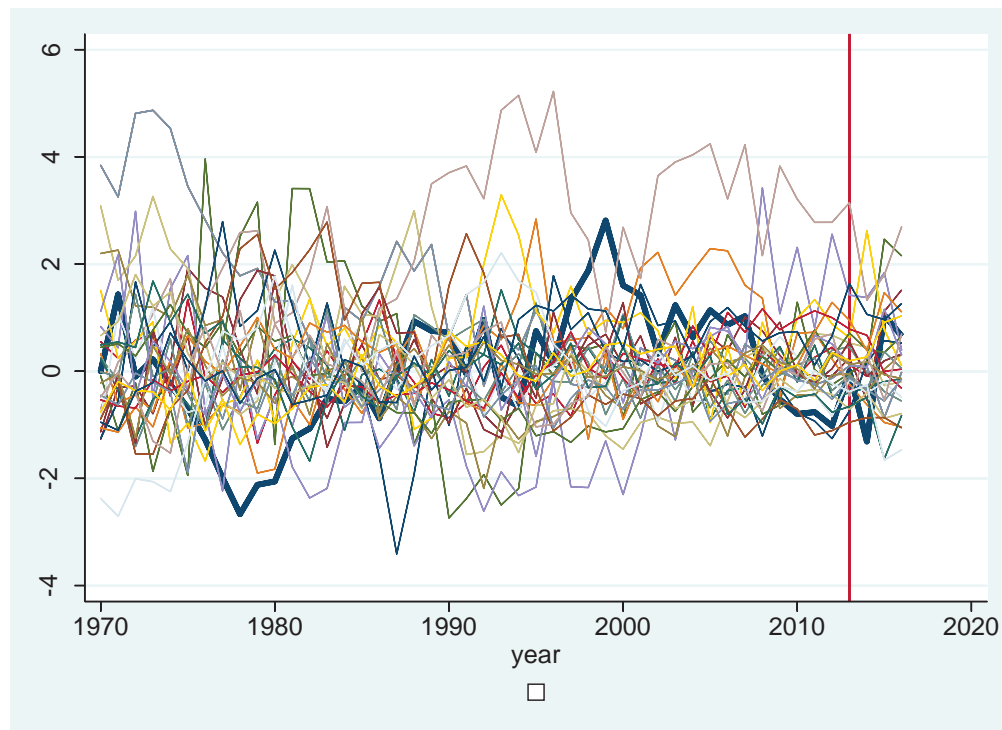
Figure 3: Maryland actual and synthetic firearm homicide rates



Maryland's average firearm homicide rate since 2013 is 6.28 per 100,000. According to the synthetic control algorithm, synthetic Maryland had an average firearm homicide rate of 6.14. This is the counterfactual estimating what would have happened to the firearm homicide rate in Maryland, if the FSA had not been implemented. Thus, the FSA has not reduced firearm homicides, despite an 82% reduction in crime guns.

One problem with the synthetic control method is that it is difficult to determine if the gap between actual Maryland and counterfactual Maryland is significantly different from zero. Abadie, Diamond and Hainmueller (2010) suggest a placebo or falsification test to address this problem. In this approach, I reassign the FSA to each of the 30 control states in turn, shifting Maryland to the donor pool. This simulates an experiment in which one of the non-permit states adopts the FSA instead of Maryland. I then estimate the effect in that state, using the same synthetic control method used to generate synthetic Maryland. Since we know that none of the control states adopted a permit to purchase law, we know that the actual effect of the law is zero. This gives us a distribution, centered on zero, of gaps between the actual firearm homicide rate and the corresponding synthetic rate for the 30 donor states. We then compare the Maryland gap with the gaps in the non-permit states. If the gap in Maryland is much higher (or lower) than in the control states, that would be evidence that the FSA had a positive (negative) effect on the firearm homicide rate. The results of the placebo test are shown in Figure 4.

Figure 4: Firearm homicide rate gaps in Maryland and all 30 control states



Notes: The thick black line is Maryland. The vertical line is 2013.

Clearly, the drop in the firearm homicide rate in Maryland in 2014 is highly unusual and would indicate a great success if it had continued. However, the huge increases in 2015-16 means that the average response since 2013 in Maryland is close to the overall average. The synthetic control analysis confirms that, overall, the FSA has failed to reduce firearm homicides.

Difference in differences method

A third alternative is to conduct “difference in differences” analysis. In this approach I choose Maryland and one control (non-permit) state for comparison. Delaware, for example, might be a reasonable choice for the control state. I then construct three dummy variables. The first takes the value one if the year is greater than 2013, zero otherwise. The second takes the unit value if the state is Maryland. The third (called the “DD” dummy) is the product of the first two, taking the unit value if the observation comes from Maryland after 2013. This is the variable of interest because its estimated coefficient tells us the difference in firearm homicide rates in Maryland compared to Delaware, after the FSA. The counterfactual is what would have happened in Maryland if it had had the same trend in the firearm homicide rate as Delaware.

Not surprisingly, given the results to this point, the coefficient on the DD dummy is not significantly different from zero.⁵ I conducted the same analysis using Virginia and Pennsylvania

⁵ The details and complete results are in the log file in Appendix B.

in turn as the control state. The result was the same. I also generated the synthetic control state used in the synthetic analysis reported above. Again the result was no significant difference between the experience of Maryland with FSA and similar states with no permit law.⁶ It is interesting, as shown in Table 1, that neither Delaware, Pennsylvania, nor Virginia have had firearm homicide rates higher than Maryland since 2013. Synthetic Maryland had higher rates in 2013-14, but considerably lower in 2015-16.

Table 1: Firearm homicide rates in Maryland and non-permit states

Year	MD	DE	PA	VA	SynthMD
2013	5.04	4.11	3.80	2.82	5.77
2014	4.17	4.06	3.64	2.83	5.48
2015	7.42	5.61	4.08	3.30	6.37
2016	7.25	4.62	4.21	4.20	6.59

Fixed-effects panel data method

A final alternative is to examine the experience of all the states that adopted a permit to purchase handgun law during the sample period to answer the following question: Has the permit to purchase law had any effect on firearm homicides in the states that have changed their laws between 1970 and 2016?

Five states adopted permit laws in the sample period: CT 1995, IA 1978, MN 1977, NE 1991, and MD 2013. A sixth state, Missouri had a permit law before 1970 but repealed it in 2008.

One of the problems of the previous counterfactual analyses is that of unobserved heterogeneity. States differ from each other in a number of ways: climate, culture, history, politics, attitude toward crime, attitude concerning law enforcement, etc. We all know that Texas is different from New York, Mississippi is different from New Jersey, Hawaii and Alaska are different from each other and from all the other states, etc. These differences are permanent, at least over the sample period, and unobservable in the sense that they cannot be properly measured. Also, these characteristics can be correlated with crime and the effectiveness of laws created to deal with crime. Therefore any cross-section analysis will suffer from an omitted variable bias that could create a spurious correlation between crime and laws passed to reduce crime. The only way to cure unobserved heterogeneity is to collect a panel of state data across several years and to use the fixed effects (FE) model to estimate the relationship between crime and crime policy. The fixed effects model corrects for unobserved heterogeneity by using only variation within each state (i.e. time series variation). In fact, statistics books frequently refer to the FE model as the “within” model. In essence, the FE model asks the following question: What has happened to firearm homicide within each state after the passage of a permit to purchase law? The result is an average effect across all states that have changed their policy by either enacting or repealing permit laws during the sample period.

⁶ I repeated the difference in differences analysis adding a lagged dependent variable to the regression. The results were unchanged. Details are in Appendix B.

I estimated four versions of the fixed effects regression. For the first two I use data on the levels of firearm homicide, adding a lagged dependent variable in the second version to correct for any unobservable effects in each state that are not permanent effects. The results are presented in Table 2. The coefficient of interest is on the dummy variable for a permit to purchase law. Consistent with our earlier results, the coefficient is not significantly different from zero. This indicates that permit to purchase laws have been generally ineffective.

Table 2 Fixed effect panel data regression: firearm homicide rate

Variable	Coeff	t-ratio	Coeff	t-ratio
Permit to purchase	-0.718	1.85	0.092	0.72
Prison population per capita, lagged	0.012	0.03	-0.023	0.21
Police officers per capita, lagged	-0.583	0.92	-0.216	1.30
Cutions	-0.022	1.15	-0.005	0.98
Crack epidemic measure	0.316	2.05*	0.132	1.49
Beer consumption per capita	0.048	2.05*	0.014	2.14*
Truth in sentencing	-0.180	0.70	-0.108	0.78
Three strikes	0.562	1.12	0.070	0.46
Right to carry	0.156	0.69	0.001	0.01
Castle doctrine	0.034	0.12	0.083	0.90
Stand your ground	0.049	0.12	0.091	0.66
State large capacity magazine ban	-0.088	0.13	0.217	0.98
Use a gun go to jail	0.117	0.48	0.018	0.16
Background check, handgun	-0.841	1.27	-0.236	1.62
Safe storage law	-0.147	0.29	0.111	0.72
Juvenile gun ban	0.386	1.03	0.273	1.63
One gun per month	-0.349	0.57	-0.050	0.33
Sat. Night special ban	0.681	0.53	-0.019	0.06
State assault weapon ban	-0.556	0.74	0.024	0.16
Background check, private sale	0.377	1.40	0.110	1.16
Real income pc	-0.170	1.24	-0.124	2.50*
Real welfare payments per capita	1.181	0.49	-0.805	0.67
Poverty rate	0.079	2.79**	0.014	0.94
Percent black	0.700	3.24**	0.169	2.91**
Unemployment rate	-0.020	0.26	-0.031	1.15
Employment per capita	0.170	1.03	0.029	0.60
Military employment per capita	0.537	0.46	0.229	0.77
Construction employment per capita	0.359	1.40	0.255	1.86
Firearm homicide rate, lagged			0.786	17.00**
N	2,270		2,264	

Notes: * $p < 0.05$; ** $p < 0.01$; also included but suppressed to conserve space: eleven demographic age groups, year dummies, and individual state trends. Complete results in Appendix B.

There is a small number of policy changes problem here (see Appendix A for more information) because only six states passed permit to purchase laws (Conley and Tabor 2011). The t-ratio corresponding to the permit to purchase law in the first regression (omitting the lagged

dependent variable) is close to two, which might make it “marginally” significant. However, because of the small number of policy changes, the standard errors are underestimated and the t-ratio is correspondingly overestimated. I subjected both the regressions reported above to a placebo law simulation. This simulation is similar to the placebo law exercise above when I attempted to determine the p-value of the difference between Maryland and synthetic Maryland. In the current application, I choose five random states to adopt permit laws in random years (the dummy variable goes from zero to one) and one random state that repeals its law (the dummy goes from one to zero) in a random year. I then do the same regression as reported in Table 2. I repeat this exercise 1000 times. Since I know that the laws are fictitious, the distribution of the coefficients and the t-ratios on the permit dummy will be centered on zero. If the t-statistic reported in Table 2 are in the tails of this distribution (2.5% in each tail), we can conclude that the coefficient is significant at the .05 level. Also, I can compute the p-value corresponding to the t-statistic reported in Table 2. It turns out that the p-value corresponding to 1.85 is 0.115 so it is not less than .10. The p-value for $t=0.72$ is 0.310. Neither of the coefficients on the permit to purchase variable are significantly different from zero. All results, including the placebo law results, are reported in Appendix B.

Because the FE model is based on the variation within the state, it is susceptible to serial correlation, a common problem for time series analysis. Serial correlation tends to lead to underestimated standard errors and inflated t-ratios. If the serial correlation is severe enough it is called a “random walk” or “unit root” and the regression could be spurious. I tested the firearm homicide rate for a unit root using a panel unit root test. The null hypothesis of a unit root was rejected for the panel, indicating the firearm homicide rate variable is “stationary”, that is, it has a stable distribution (and the regressions in Table 2 are not spurious). However, when I tested each state for a unit root, 41 out of 50 states indicated the presence of a unit root. Thus, the evidence is conflicting on whether the firearm homicide rate is a unit root process or not.

A simple cure for the unit root problem is to take first differences (the changes in the values of the variable from each year to the next). So, as a robustness test, I estimated the same models as shown in Table 2, but using first differences. The results are presented in Table 3.

Table 3: Fixed effects model: firearm homicide rate, first differences

Variable	Coeff	t-ratio	Coeff	t-ratio
Permit to purchase	-0.005	0.01	-0.035	0.11
Prison population per capita, lagged	0.215	1.26	0.221	1.26
Police officers per capita, lagged	0.050	0.49	0.053	0.51
Executions	0.035	3.19**	0.036	3.25**
Crack epidemic measure	-0.006	2.72**	-0.006	2.50*
Beer consumption per capita	-0.000	0.14	-0.000	0.23
Truth in sentencing	-0.009	3.03**	-0.009	3.28**
Three strikes	0.006	0.05	0.004	0.04
Right to carry	0.147	0.85	0.156	0.92
Castle doctrine	0.075	0.41	0.086	0.47
Stand your ground	-0.008	0.04	-0.021	0.12
State large capacity magazine ban	-0.531	3.19**	-0.532	3.27**
Use a gun go to jail	0.003	0.02	-0.008	0.04
Background check, handgun	-0.131	0.39	-0.148	0.43
Safe storage law	-0.046	0.16	-0.058	0.19
Juvenile gun ban	0.280	1.54	0.288	1.60
One gun per month	-0.098	0.31	-0.130	0.40
Sat. Night special ban	-0.116	0.47	-0.124	0.49
State assault weapon ban	0.128	0.45	0.131	0.45
Background check, private sale	0.299	1.15	0.312	1.13
Real income pc	-0.202	2.38*	-0.206	2.37*
Real welfare payments per capita	-0.402	0.25	-0.393	0.25
Poverty rate	0.001	0.04	0.002	0.13
Percent black	0.462	2.16*	0.492	2.35*
Unemployment rate	0.008	0.30	0.014	0.51
Employment per capita	0.024	0.37	0.027	0.43
Military employment per capita	0.001	0.00	0.037	0.08
Construction employment per capita	0.749	4.01**	0.749	4.03**
Firearm homicide rate, lagged			-0.059	0.82
N	2,212		2,211	

Notes: * $p < 0.05$; ** $p < 0.01$; also included but suppressed to conserve space: eleven demographic age groups, year dummies, and individual state trends. Complete results are shown in Appendix B.

The results are the same, namely that the coefficient on the permit law dummy is not significantly different from zero in either regression. Complete results, including placebo law simulations, are presented in Appendix B.

Summary and conclusions

I have estimated a variety of models in an attempt to determine if the Maryland FSA law has had the expected beneficial effect on the firearm homicide rate, the bell weather of gun crime. I find that the firearm homicide rate has increased more in Maryland after the FSA than the average of all the non-permit states. The synthetic control model (Abadie, Diamond, and Hainmuller 2010)

also shows that the firearm suicide rate increased more in Maryland after the FSA than it did in the synthetic version of Maryland. In addition, difference in differences analysis of Delaware, Pennsylvania and Virginia, nearby non-permit states, indicates that Maryland did not have an increase significantly less than these control states. Similarly, a difference in differences analysis of the synthetic control state, formed as a weighted average of three non-permit states, indicated that gun murder increased no less in Maryland than in the synthetic control state. At best, the FSA law had no significant effect on firearm homicide.

These results were confirmed with a panel data fixed effects regression model that estimates the effect of permit to purchase handgun laws in general. Six states have changed their policies with respect to such laws in the sample period. I estimated the model four ways. In all four cases, the results were the same, namely permit to purchase handguns laws have had no significant effect on the firearm homicide rate.

Given Professor Webster's impressive 82 percent reduction in crime guns achieved by the FSA, one might be forgiven if one assumes that this would lead to a significant reduction in firearm homicides. However, the consensus finding of the analyses reported here is that the FSA is associated with an increase in firearm homicides, albeit an insignificant increase. I conclude that the FSA has had no beneficial effect on firearm homicide. In Maryland, gun control has not translated into crime control, at least with respect to the FSA. Apparently the supply of crime guns has no effect on the supply of gun crime.

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Appendix A: A primer on statistical methodology

A test to determine if two means are significantly different from each other is called a “t-test.” The test statistic is the difference between the two means divided by the standard error, a measure of the variation or variance of the data. The test assumes that the data follow a normal distribution. For a test of the difference between two means the test statistic is as follows where the observed mean for Maryland is $x=20$ and the overall mean for the non-permit states is $x_0=10$.

$$t=(x-x_0)/S_x=(20-10)/12=10/12=.83$$

S_x is the standard deviation of x , the usual measure of variance for t-tests. If x is highly variable, a difference of 10 would not be unusual. However, if the variance of x is small, then a 10 percent difference would be unusual (“significant”).

Statistical tests are formulated around two hypotheses. The “null hypothesis” assumes no difference between the means while the alternative assumes that there is a difference, which could be positive or negative. Since the two means could differ from each other due to random chance, we have to decide how big a difference is significant. That is, how large must the difference be to reject the null hypothesis of no difference (H_0) and accept the alternative hypothesis of a difference, which could be positive or negative (H_a). For this we use a “p-value.” It turns out that, if the null hypothesis is true, it is very unlikely to find a value for the test statistic if the difference is more than two standard errors away from zero. The p-value for this value is .05. That is there is less than a five percent chance that the percent change of the firearm homicide rate is just randomly higher than the overall mean.

The value of the t-ratio in the above example is .83, so the difference is less than two standard errors away from zero. The p-value is 0.4, which is greater than .05, so we cannot reject the null hypothesis that the mean is 10, that is, Maryland’s mean of 20 is not significantly different from the mean of the non-permit states.

The choice of $p=.05$ is traditional, but it is also intuitive. Suppose you are playing a simple coin tossing game against the computer in a casino. Suppose for simplicity that the casino does not take a cut. You are playing the \$100 game. You win \$100 if the game shows tails and you lose \$100 if heads appears.

First play: heads. Do you want to play again? Yes.

Second play: heads. Do you want to play again? Yes.

Third play: heads. Do you want to play again??

Fourth play: heads. Do you want to play again?

When do you decide that the game is rigged? The probability of heads on the first play is .5. The probability of two heads is .25. Not that unusual. The probability of three heads is .125. Thinking of quitting? If you play one more time, you are almost certain that the game is rigged. The

probability of four heads is .0675, which is close to .05. A p-value of .10 (approximately the probability of three heads) is called “marginally significant” and is sometimes reported. I do not use that criterion, preferring to hold significance tests to the .05 standard.

An alternative way of determining significance is to see if the test statistic (the ratio of the difference between the two means divided by its standard error, $t = (x - x_0) / S_x$) is greater than the “critical value.” The critical value is the value the appropriate distribution, usually the t-distribution, a slight modification of the usual bell-shaped curve or normal distribution. It turns out that the critical value corresponding to .05 is approximately equal to 2. Therefore, a t-statistic is significant if its absolute value (since it can be positive or negative) is greater than 2, which would imply that its p-value is less than .05.

A regression model such as the fixed effects panel data model used to estimate the effect of permit to purchase laws across all states, estimates the possible linear relationship between the dependent (outcome) variable and a policy or law variable. The law variables are so-called “dummy” variables which take the value one in those years the law was in effect, zero otherwise. I also include trends for each state consisting of the numbers 1,2,3, etc. for the years in the sample. The coefficients on the trends show by how much the murder rate changes each year due to all other factors that affect the murder rate aside from the variables included in the regression model. These factors include changes in trauma treatment that turn potential murders into assaults, the advent of 911 calls, cell phones, DNA, the national fingerprint directory, ubiquitous security cameras, smartphones with cameras, body cameras on police officers, etc. etc. If the trend is omitted, these influences on crime which are separate and distinct from the effect of any law, will be incorrectly attributed to the law variable. I also include a number of variables that are routinely included in crime models such as police officers per capita, the incarceration rate, the unemployment rate, income per capita, other laws, etc.

The coefficient on the law dummy variable estimates the change in the dependent variable, e.g., the gun murder rate, due to the implementation of the law, holding constant the effects of the control variables. If the law has been effective in reducing gun murder, we would expect a negative coefficient on the law dummy variable indicating a reduction in gun homicide as a result of the law.

Even if an estimated coefficient is negative, it does not mean the law necessarily had a beneficial effect. If the law had no effect, the coefficient on the law dummy variable could be negative just by chance. In fact, we would expect it to be negative 50 percent of the time. How do we know when an estimated coefficient is significantly different from zero? We use the t-test again, this time testing the difference between the estimated coefficient on the law dummy and zero (indicating no effect). The resulting t-statistic is just the ratio of the estimated coefficient to its standard error.

As in the case of the t-test above, the larger the value of the estimated coefficient, the more likely that it is not zero. However, given the standard error, we would expect some variation around zero even if the true value is zero (i.e., the null hypothesis is true). If the estimated coefficient is distributed according to the normal distribution, the usual assumption, then it would be quite

unusual for an estimated coefficient to be twice as large as its standard error (t-ratio greater than 2). As noted above, this would only happen 5% of the time if the true value of the coefficient was zero. Therefore, we reject the null hypothesis that the FSA had no effect if the absolute value of the t-statistic is greater than two, or equivalently, if the p-value is less than .05.

With respect to the panel data analysis using dummies for permit to purchase handgun laws (permithg), there are only six states that adopted such laws during the sample period, from 1970-2016. Conley and Tabor (2011) show that fixed effects regressions using a small number of policy changes (six states with policy changes is small) will underestimate the standard errors and overestimate the t-ratio corresponding to the policy dummy variable. This makes the coefficient appear to be significant when it is in fact insignificantly different from zero. One cure for this approach is to use a “placebo law” simulation to determine the correct critical value, since the underestimation of the standard errors means that a t-ratio larger than two will occur randomly with a higher than .05 probability. This procedure was suggested in a famous article by Bertrand, Duflo and Mullainathan (2004). In a placebo law program, I assign a the value 1 to a dummy variable in random states in random years. The laws these dummy variables represent are completely fictional, so we know that the effect of the laws on the firearm homicide rate is exactly equal to zero. Nevertheless, if I repeat the procedure 1000 times, some of the 1000 estimated coefficients on the law dummy will be significant simply by chance. The corresponding 1000 t-ratios will form a distribution centered on zero. I can find the critical value for the t-ratio by finding the 25th and the 975th value of the t-ratio from the list of all 1000 t-ratios. (I use the 975th value if the coefficient is positive and the 25th value if the coefficient is negative. This reflects the fact that the test is “two-sided” so that the coefficient could be either positive or negative under the alternative hypothesis. I therefore have to put 2.5% of the distribution in each tail.)

Cross-section studies, those that use, for example, states in the US in a given year, suffer from unobserved heterogeneity. This is a kind of omitted variable bias in which the estimated coefficient is biased because the variance of the dependent variable is due in part by unobserved fixed factors and the cross-section model has to omit such factors because they are not measurable. We know that Massachusetts and New Mexico are different in a number of ways: climate, culture, history, tradition, etc. These factors are permanent and not easily measured. However, they could affect the relationship between gun homicides and laws designed to reduce gun homicide. It is only possible to control for such permanent unobservable factors by using a fixed effect model. This model uses the variation within each state (before and after the implementation of the law) to determine if the law had an effect.

Synthetic control methods vs fixed effects models

This approach attempts to estimate the counterfactual outcome of the treated states by matching the pre-treatment experience of the treated states with those of the control states. The difference between the counterfactual post-law crime rate and the actual crime rate in the treated states estimates the effect of the law. The counterfactual is estimated by a weighted average of the outcome variable, Y_{it} , with positive weights summing to one and re-weighted over time to minimize the mean square prediction error of the pre-intervention outcomes in the treated states

(Abadie et al 2010). Under certain assumptions the resulting difference between the actual and counterfactual outcomes of the treated states is an unbiased estimator of the average treatment effect on the treated state (Abadie et al, 2010, p. 496). This approach will control for unobserved time-varying effects (see next paragraph). But, because it is an average across the control states, it is therefore biased in the presence of fixed effects.

Another approach is called the lagged dependent variable (LDV) method. This approach uses the lag of the outcome variable to control for any time-varying unobservable factors that led the state to pass a law in the first place. The lag dependent variable is the history of the outcome variable, gun murder in this case, which captures the state's experience with gun murder. That experience could affect whether the state adopts a permit law or not. The LDV method, however, is also biased in the presence of fixed effects.

I am aware of only one study that directly compares the fixed effects, lagged dependent variable, and synthetic control methods (O'Neill, et al, 2016). The authors find that the LDV models performs best overall among these three. They conclude (p.17).

The LDV approach returns more efficient estimates than the synthetic control approach, while also further mitigating bias. We conclude that the LDV approach is an attractive estimation approach in this setting...

An approach that estimates a LDV model yet controls for unobservable heterogeneity would appear to be a superior alternative. Such an alternative is possible if we combine the FE and LDV models by adding a set of lagged dependent variables to the standard FE model, thus controlling for both fixed and time-varying unobservables.

$$(5) \quad Y_{it} = \alpha_i + \sum_{j=1}^J \eta_j Y_{i,t-j} + \delta_i t + \beta_1 D_{it} + \gamma' X_{it} + \lambda_t D_t + \varepsilon_{it}$$

This method suffers from the Nickell bias (Nickell 1981). However, the Nickell bias is primarily a problem for the lagged dependent variable and is a function of the number of time series observations. I have tested this model using Monte Carlo simulations. There is virtually no bias in the estimates of the coefficients on the policy dummy for samples with more than 45 observations, as we have here.

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Appendix B

Documentation for opinions and analyses in the main report

Figure1 and Figure 2 are generated using Figure1.do and Figure2.do. (P.19)

The t-test of the difference of Maryland's increase in firearm homicide rate after 2013 compared to the increase in non-permit states is generated using t-test.do and the results are in t-test.log. (P. 19)

To conserve space, I have included only the log files, which have both the commands in the do file and the output generated by those commands. However, the data and do files can be supplied if necessary.

The difference in differences analyses are done using dd.do and reported in dd.log. (P. 21)

Documentation for the fixed effects panel data analyses reported in Table 3 are presented in gun.murder.cluster.general.log (P. 28)

gun.murder.cluster.LDV.log (P. 37)

Documentation for the first difference analyses reported in Table 4 are in fd.gun.murder.cluster.general.log (P. 48)

fd.gun.murder.cluster.LDV.log (P. 71)

Documentation for the synthetic control analysis reported in Figure 3 is in:

synth.MD.log (P. 94)

The placebo analysis that produced Figure 4 is documented in

synth.placebo.log (P. 98)

Figure4.log (P. 212)

**PAGES 19-213 WHICH CONSIST OF THESE DATA RUNS
HAVE BEEN OMITTED**

Exhibit 1

Curriculum Vita of Carlisle E. Moody

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Education

B.A., Colby College, Waterville, Maine, 1965 (Economics)
M.A., University of Connecticut, Storrs, Connecticut, 1966 (Economics)
Ph.D., University of Connecticut, Storrs, Connecticut, 1970 (Economics)

Experience

Professor of Economics, College of William and Mary, 1989-
Chair of the Department of Economics, College of William and Mary 1997- 2003
Associate Professor of Economics, College of William and Mary, 1975-1989.
Assistant Professor of Economics, College of William and Mary, 1970-1975.
Lecturer in Econometrics, University of Leeds, Leeds, England, 1968-1970.

Consultant

Stanford Research Institute
Virginia Marine Resources Commission
U.S. General Accounting Office
U.S. Department of Transportation
U.S. Department of Energy
National Center for State Courts
Oak Ridge National Laboratory
Justec Research.
The Orkand Corporation
Washington Consulting Group
Decision Analysis Corporation of Virginia
SAIC Corporation
West Publishing Group
Independence Institute

Research and Teaching Fields

Law and Economics
Econometrics
Time Series Analysis

Honors

National Defense Education Act Fellow, University of Connecticut, 1965-1968.
 Bredin Fellow, College of William and Mary, 1982.
 Member, Methodology Review Panel, Prison Population Forecast, Virginia Department of Planning and Budget, 1987-1993.
 Notable Individuals, Micro Computer Industry, 1983.
 Speaker, Institute of Medicine and National Research Council Committee of Priorities for a Public Health Research Agenda to Reduce the Threat of Firearm-related Violence, National Academies of Science, Washington, DC, April 23, 2013

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